the opinions of those who would affirm that indicanuria had any causal relation to mental disease, were based upon insufficient examination and errors due to a failure to take into account the effects of ordinary constipation in those not affected mentally. (Vide abstract of *Journal of Medical Science* in the issue for January, 1908.)

Cotton (Trenton).

## Journal de Neurologie

(1906, 16, 17, 18, 19)

These numbers are devoted to the proceedings of the Second Belgian Congress of Neurology and Psychiatry.

1. Presidential Address. "Neurasthenia." Crocq.
2. The Theory of the Neurone in the Last Decennial Period (1896, 1906). M. Stefanowska.
3. Mental Tests as Applied to the Child. Decroly and Boulenger.
4. Lunatics Who Dissimulate. Moor and Duchateau.

1. *Presidential Address: "Neurasthenia."*—Calling attention to the confusion which exists in the general conception of neurasthenia, held by the medical profession and laity alike, the author pleads for greater accuracy in separating the different nervous states, and especially for the differentiation of functional conditions from those symptomatic of organic disease. For him neurasthenia is a definite disease, not merely a symptom-complex, and at the start he rules out the great array of degenerates who show the various phobias, aboulia, etc. In his opinion a hereditary constitutional basis is not necessary but the disease may arise as a result of exhaustion from excesses, intoxications or any other cause. He gives a short sketch of the symptoms of this disease and its diagnosis from general paresis, dementia præcox and other conditions in which symptoms of irritable weakness of the nervous system are present. He thinks that the term "neurasthenic states" is more or less objectionable but considers these as comprising: (1) True neurasthenia, an autonomous curable disease. (2) Constitutional neurastheniform states against which our therapeutic resources are often powerless. (3) Neurastheniform symptoms accompanying certain organic diseases, and whose prognosis varies according to the nature of these diseases.

2. *The Theory of the Neurone.*—In a critical review of 75 pages the authoress considers, in order, the work of Apáthy, Bethe, Held, Auerbach, Nissl, Joris, Donaggio and others, opposing the neurone theory, and that of Ramon y Cajal, of Van Gehuchten and his pupils, of Lenhossék, Marinesco, Lugaro and others, in its support. She then proceeds to a general discussion of the subject, examining one by one the following postulates of the partisans of the theory: (1) The neurone is an embryological unity. (2) The neurone is a cellular and anatomic unity. (3) Besides the neurone there is no other nerve element. (4) The neurones are in relation to each other by contiguity. (5) The neurone is a trophic unit. (6) The neurone is a functional and physiological unit. The embryological unity of the neurone she thinks severely shaken by the results of the researches of a number of authors which seem to show, on the one hand, that the nerve fiber is developed by the apposition of a number of cells in a chain, and on the other by the work on the autoregeneration of the nerve fiber by Bethe, Monkeberg, Merzbacher, Ballance and Stewart, and Van Gehuchten. The results of these authors have, however, been disputed, and in

some experiments of her own, on very young white mice, the authoress could not convince herself that the nerve fiber was ever developed from more than one cell. The ardently disputed question of continuity or contiguity, is considered at length, and while giving to Apáthy and Bethe full credit for the discovery of the neurofibrils, since amply confirmed by the newer researches of Ramon y Cajal and others, by the Spanish histologist's new and easily applied fibril method, and while acknowledging that in invertebrates the relation of continuity exists, she maintains that in the case of the higher animals these authors have failed to demonstrate such a relation, but have concluded that it exists here also, simply on the strength of what they have observed in invertebrates. The results of Ramon y Cajal, and others, are directly opposed to the idea of continuity, for, while the plexuses of neurofibrils have been clearly enough demonstrated, in no case could anastomosis of fibrils from different neurone systems be found. As to the network surrounding the nerve cells which has been called by Bethe the "Golgi network," the fact of its nervous character is disputed, being denied by Golgi himself. The pericellular network of Held and his "neurosomes" (or "end feet") have been confirmed by other observers, especially by Ramon y Cajal working with his new silver method, and that these are nervous structures there seems little doubt. No one, however, has found that there is any passage of fibrils from the pericellular network *into* the ganglion cells and the end feet have never been seen to separate the cell wall. This relation is made especially clear by the silver method of Cajal. Hence, at any rate, as relating to vertebrates, the idea of relation by contiguity does not seem to the authoress to be shaken. As stated above, the experiments upon the regeneration of the peripheral nerves has thrown considerable doubt upon the trophic unity of the neurone. The interpretation given to these results has, however, been strongly disputed by Dejerine and others who claim that in the experiments upon which they are based, the possibility of the connection of the cut nerves with the spinal cord centers through the medium of branches from some other nerve was not eliminated beyond all doubt. As to the physiological and clinical unity of the neurone, the authoress shares the views of those who hold that even if the idea of the anatomical unity had to be modified, nothing has so far been brought out which forces us to abandon our view of the essential unity of the neurone in physiological function. What we have observed of the course of the nerve impulse and the reflexes speaks in favor of this, and no one has yet been able to show that degeneration passes over from neurone to neurone. She finally takes up the theory of this ameboism of the nerve cells of Mathias Duval, and in this connection gives a résumé of some investigations with regard to the pearls on the dendritic processes carried on by herself and published in 1897.

In order for this theory to be correct it would be necessary to demonstrate that the prolongations of the nerve cells are really capable of elongating, that the formation of these pearls is due to the retraction of these prolongations, that the pearls appear and disappear rapidly like the pseudopods of the ameba with which the prolongations of the nerve cell have been compared. The authoress thinks that there has been a confusion of the normal and the pathological morphology of the neurone on the part of those who have sustained the theory of ameboism, and who seem to have become possessed of a false idea with regard to the rôles respectively of the pearls or, as she prefers to call them, "pyriform appendices," and of the moniliform or varicose condition of the dendrites. She has studied the nerve cell and its processes by the various modifications of the chrome-silver method as well as by the vital methylene-blue methods and finds that the pyriform appendices are demonstrable by each of them, hence they can hardly be considered as artefacts. That both they and the dendritic process proper are modified in disease and in intoxications she also shows. The dendrites undergo varicose or moniliform change while the pyriform appendices may disappear, these changes being more or less independent of each other. In animals subjected to the action of strong electric currents, and in those kept under prolonged narcosis, similar changes have also been found. In sections prepared by the fibril method of Cajal, the pyriform appendices are not visible, hence they are probably protoplasmic structures. In the pathological conditions studied they seem in some way absorbed by the dendrite on which they are implanted. In the moniliform condition too, the neurofibrils escape, the protoplasmic sheath alone being affected. The authoress has never found retraction of the dendrites in the moniliform condition and thinks that we have not to do here with a "contraction of the protoplasm" but with an abnormal liquefaction. Hence the idea of a "Morphological plasticity" or ameboism of the neurone as based upon this moniliform state is considered by her as a gross error. A list of over two hundred references is appended.

3. *Mental Tests as Applied to the Child.*—In a very comprehensive report covering over one hundred pages, the authors consider the application of psychological tests to children. In their opinion, mental tests should take in both observation and experience, should be short and easy enough to be used as a routine practice, and exact enough to give reasonably constant and comparable results. Since the aptitudes of mankind are variable and unequally distributed, they think that the questions to be answered, if possible, are, on the one hand, what special aptitudes are necessary for any given professional work, on the other, what particular aptitude does a given child possess? Dividing their report into two parts, in the first they pass in review the various tests proposed by different authors and it here appears that the schemes for testing will vary considerably, depending upon whether they are to be applied to normal or to defective children. In the second part of their report, they consider the results of mental tests as applied to very young children, beginning with such observations upon anencephalous monsters as have been published from time to time, next considering such as have been made upon prematurely born infants, and so on up to children aged three years. To a general statement of results obtained as set forth in the literature, they add a short résumé of their own experiences in series of eighteen cases. Into their methods, as detailed, and into the conclusions which they draw, it is impossible to enter in a short abstract. Suffice it to say that for children from birth to the age of three years, they propose a series of simple tests relating to taste and ability to select aliment, to cutaneous and articulo-muscular sense, to vision (with motor reaction), to hearing (with motor reaction) and language, to skill in executing movements, to sociability, playfulness, sentiments, etc. Those specially interested in the subject will find in this report a number of valuable suggestions, and should consult it in the original.

4. *Lunatics who Dissimulate.*—The author thinks that the question of dissimulation deserves attention quite as much as that of simulation,

since many lunatics, for various reasons, will try to cover up their condition. After giving short histories of some cases illustrating this symptom as found in defectives, sexual perverts, chronic alcoholism, paranoia and manic-depressive insanity, they draw the following conclusions: (1) Dissimulation requiring a certain cerebral tonus, and relative integrity of judgment and will, can only exist in those psychological states in which the syllogistic faculties are preserved or in the period of remission of other psychoses. (2) Dissimulation is found in different kinds of insanity, but specially in melancholia, paranoia and mental degeneration. (3) Dissimulation has a greater importance than simulation, especially in the case of lunatics demanding their discharges. (4) The diagnosis of dissimulation requires prolonged observation preferably in a hospital or asylum.

C. L. Allen (Los Angeles),

## Journal de Neurologie

### (No. 1. 1907)

1. Psychic Imitation, Normal and Morbid. Marandon de Montyel.
2. Application of the Diazo-reaction to the Prognosis in Status Epilepticus. Masoin.

1. *Psychic Imitation, Normal and Morbid.*—A discussion of the mechanism of psychic imitation, whether it is of endogenous or of exogenous origin. The author, a partisan of the origin of the emotions from within, regards their expression through gestures, etc., as secondary to the mental process at the base of the affective state, and not as the primary source of this state. He supports his position with his usual vigor, but into his arguments it is impossible to enter in a short abstract.

2. *Application of the Diazo-reaction to the Prognosis in Status Epilepticus.*—The author gives a résumé of the conclusions arrived at, in a mémoire recently presented by him to the Academy of Medicine of Belgium. He expressly affirms that he does not claim that the conditions giving rise to the diazo-reaction are the cause of epileptic attacks. There is simply a chronological relation. He was able to apply the test for this reaction in 16 out of 35 cases of status epilepticus. Of these patients, 19 died while 16 survived. In the 16 cases examined for the diazo-reaction, a positive result was obtained in 9 cases, a negative in 7 cases. Of the 9 cases showing a positive reaction 6 died, 3 survived. Of the 7 cases with negative reaction, one died, 6 lived. He hence feels justified in concluding that the presence of the diazo-reaction in status epilepticus is distinctly an unfavorable sign, its absence, on the contrary, favorable. These indications are, however, only relative since, while the absence of the reaction is in general a favorable sign, its presence does not necessarily imply a fatal issue.

### (No. 7. 1907)

1. *The Relation Between Dipsomania and Chronic Alcoholism.* Soukhanoff.

Considering this condition as he has observed it, the author feels justified in drawing the following conclusions: (1) Typical dipsomania develops after antecedent alcoholism, occasional or chronic. (2) It shows itself by preference during middle life. (3) It appears as one of the phases of long continued abuse of strong drinks. (4) In cases of dipsomania the use of alcoholic beverages has, according to the case history, usually been begun early, even in adolescence.